

## Correspondence

EDITED BY STANLEY ZAMMIT

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### High-security hospitals<sup>†</sup>

I have been invited to respond to Dr Exworthy and Professor Gunn's critique of the review of security at the high-security hospitals (Exworthy & Gunn, 2003, this issue). Our report (Tilt *et al*, 2000) made 86 recommendations, all of which were accepted by the Government.

As I read the critique the main argument is that the team ignored the importance of relational security and was too preoccupied with physical and procedural security. I think this is a serious misinterpretation. The authors do not appear to have taken sufficient account of section 2 of the report, specifically paragraph 2.5, in which we said:

'In the view of the review team it is important that patients feel engaged and committed to the hospital. The provision of a full and purposeful activity and therapy programme is essential both for treatment purposes and as a significant part of the creation of a secure and safe environment. In the same way, the review team believes that beyond specific individual and group therapy it is important for a patient's daily life to be as active and demanding as possible having regard to the constraints of individual illness/disorders. It is for this reason the review team's recommendations have two main thrusts

- an increase in therapy and activity for patients
- an upgrading of physical and procedural security to safeguard the public, staff and patients' (Tilt *et al*, 2000: p. 5).

The Faulk (1985) formula for a successful secure unit cites: (a) sufficient physical security appropriate to the patients; (b) high staff ratios; and (c) a therapeutic policy which encompasses individual programmes.

In my view this does not go far enough. Providing high staff ratios offers very little unless the staff are properly trained, motivated and managed. One of the shortcomings we found in the three hospitals was that although good therapy, expertise and

resources were available, they were significantly underused because there was little or no management information or action to ensure that the best possible outcomes were achieved from the resources made available.

In terms of the specific criticism that we neglected relational security, it is worth recording that recommendations 7, 15 and 57 related specifically to this aspect. The authors also assert that there was no clinical member on the enquiry team. This is not correct – one member of our team had extensive clinical experience, including working in high-security hospitals. Beyond that, in each of the three hospitals we spent time consulting many clinicians, including psychiatrists, and were struck by how many suggested to us that the existing security arrangements at that time were inadequate.

I believe firmly, as did all the members of my team, that the key to running successful treatment-oriented high-security hospitals lies in ensuring that the public, patients and staff feel safe about their operation. I believe our recommendations are making a positive contribution to that.

**Exworthy, T. & Gunn, J. (2003)** Taking another tilt at high secure hospitals. The Tilt Report and its consequences for secure psychiatric services. *British Journal of Psychiatry*, **182**, 469–471.

**Faulk, M. (1985)** Secure facilities in local psychiatric hospitals. In *Secure Provision* (ed. L. Gostin). London: Tavistock.

**Tilt, R., Perry, B., Martin, C., et al (2000)** *Report of the Review of Security at the High Security Hospitals*. London: Department of Health.

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### How should advance statements be implemented?

Papageorgiou *et al* (2002) are quite right to point out that advance directives (or advance statements) have potentially

beneficial effects on the processes of care, but the best way of implementing and evaluating them is far from clear. They chose a randomised controlled trial (RCT) to evaluate the effectiveness of advance statements, and used the number of compulsory admissions a year later as the main outcome measure. They found that advance statements had little impact on the outcome of care.

Different research methodologies exist to answer different types of research question, and while RCTs may be appropriate for establishing the effectiveness of an intervention, they provide little information as to the best way of implementing and delivering an intervention, especially complex interventions such as advance statements, which serve the ethical purpose of trying to preserve individual autonomy. In view of this we have to consider the power relationships between service users and mental health services. The authors appear to be aware of these relationships. The booklets in which their patients wrote their directives clearly stated that patients' wishes could be overridden by compulsion. This raises many questions.

Who 'recruited' patients into the study? How did recruitment take place? What steps were taken to inform service users about the pros and cons of advance statements? How were service users, professionals and structures of care such as the Care Programme Approach process prepared for advance statements? These questions concern power, values and interest. Do professionals really consider advance statements to be helpful, and take their implementation seriously? If not, how might this affect the way patients respond when asked whether they want to write an advance statement? The discussion in the paper indicates that staff may have had a 'lack of sustained awareness' of advance statements over the follow-up period. Our experience in Bradford indicates that a considerable amount of developmental work with mental health professionals and service users is necessary if advance statements are to be implemented.

The Medical Research Council (2000) has prepared a framework for use of RCTs for complex interventions, which sets out four stages of development. It starts with pre-clinical justification for the intervention, followed by modelling (defining the intervention and understanding the relationships between the component parts), and concluding with long-term

<sup>†</sup>See editorial, pp. 469–471, this issue.

implementation of the intervention. A definitive RCT should take place only after the first two stages have been completed. Our experience in Bradford is that advance statements are complex interventions that require lengthy developmental work if they are to stand a chance of success. Papageorgiou *et al* make no reference to what, if any, developmental work took place before the introduction and evaluation of advance statements, making it difficult to draw conclusions about their effectiveness or otherwise.

#### Declaration of interest

P.T. is a grant co-holder with the Mental Health Foundation in the Advance Statement Project in Bradford, funded through section 64 funding by the Department of Health.

**Medical Research Council (2000)** *A Framework for Development and Evaluation of RCTs for Complex Interventions to Improve Health*. London: MRC Health Services and Public Health Research Board.

**Papageorgiou, A., King, M., Janmohamed, A., et al (2002)** Advance directives for patients compulsorily admitted to hospital with serious mental illness. Randomised controlled trial. *British Journal of Psychiatry*, **181**, 513–519.

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**Authors' reply:** Dr Thomas is right to point up the difficulties of evaluating advance directives in mental health care. To answer their specific queries: (a) Who recruited patients? A psychologist (A.P.) and a psychiatrist (Anis Janmohamed) recruited the patients. (b) How did recruitment take place? The ward managers, responsible psychiatric nurses, junior doctors or consultants (depending on who was available at the time) were approached on a weekly basis and a list was drawn up of all patients who were near discharge from section. A.P. and A.J. introduced eligible patients to the trial and gave them a written summary of our aims and procedures. Patients were given time to read the summary and decide whether they wanted to participate in the study. Those who agreed undertook a baseline assessment and were randomised into the experimental and control group. (c) What steps were taken to inform the service users about the pros and cons of

advance statements? The participants were seen individually by A.P. and A.J., who informed them about the advantages and disadvantages of advance directives. Participants were also informed about accessibility of their local service users' groups for further advice on any related issues. (d) How were service users, professionals and structures of care such as the Care Programme Approach process prepared for advance statements? The lead academic (M.K.) had extensive discussions with managers, consultant psychiatrists and nurse managers about the study to ensure they were fully informed and prepared for the trial. Although it would have been useful to incorporate the directives into the formal Care Programme Approach process, clinicians did not think that this was warranted at that stage. Local service users' groups were informed about the study, and A.P. and M.K. talked to the groups regularly throughout and after the trial. M.K. leads a collaborative group in north London between service users and academics to promote user-led research. We considered it a strength of our trial that participants prepared their directives with someone who was *not* involved in their care, as this made the whole process less open to duress. (e) Do professionals really consider advance statements to be useful and take their implementation seriously? Professionals certainly took the intervention seriously at meetings and presentations where the study was discussed and readily agreed to the trial. However, by the end of the trial they were unsure about the value of the directives, a finding that we discuss in a further paper that has been submitted for publication (further details available upon request). (f) Was there developmental work before the introduction and evaluation of advance statements? Considerable work with users and professionals was carried out before the trial commenced to develop the format of the advance directive. However, as Dr Thomas will know, obtaining funds for this valuable work is extremely difficult, and thus it was limited. During our developmental work, we became more aware of the legal complexities of advance directives and the possibility that they could be considered binding on clinicians. Because their worth was at this stage unproven, we took the step of including a clause stating that users' wishes could be overridden. We concur with Dr Thomas's views on the Medical Research Council's framework for the evaluation of complex

interventions. However, when our study was conceived in 1996 these recommendations were not available. The pre-clinical justification for the study was increasing use of advance directives in this country and in the USA. Given the mood of the time, our study was justified.

We made it clear in our paper that we did not consider our study definitive. We would welcome further research on the additional matters raised and hope our study stimulates such work. We acknowledge that our study does not evaluate the effectiveness of advance directives under optimum conditions – in fact, that was not our aim. Ours was a pragmatic trial in which we sought to assess whether such directives were useful in a real, inner-city clinical setting. We used rates of compulsory readmission as our main outcome measure to test one bold claim made for them, namely that they may reduce the need for patients to be civilly committed at a later time. If substantiated, this is a very important matter.

Advance directives may be a useful expression of patient autonomy and self-direction. We look forward to reading the results of further research.

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#### Rivastigmine and QT interval prolongation

Walsh & Dourish (2002) reported that a 78-year-old man, receiving a number of medications and with a history of myocardial infarction and hypokalaemia, developed an abnormal QTc interval a week after starting rivastigmine treatment. I have performed an extensive review of the tolerability and safety of cholinesterase inhibitors (Inglis, 2002), in which I described the favourable cardiac safety profile of rivastigmine. Therefore, I contacted Novartis for more information. This case, which was initially submitted to the authorities in June 2001, included further clinically relevant information.

Primarily, the patient's pre-rivastigmine QTc (3 weeks before starting treatment) was 431 ms rather than 397 ms as suggested by Walsh & Dourish (C. Videbaek (Novartis), personal communication, 2002). The reported QTc of 397 ms was obtained a week after starting rivastigmine

treatment, indicating that during this week the patient's electrocardiogram (ECG) 'normalised'. The following week (2 weeks post-rivastigmine) it increased to 477 ms. The QTc prolongation (pre-rivastigmine to 2 weeks post-rivastigmine) was less than 11%. Nevertheless, since this change was above the 30 ms usually considered relevant, it is important to assess in an unbiased manner whether it was drug-induced.

The patient was already at risk of cardiac abnormalities owing to: previous increased QTc; hypokalaemia (a risk factor for QTc change; De Ponti *et al*, 2002) 2 weeks before starting rivastigmine treatment (no potassium values were reported at the time of the ECG finding); concomitant use of diltiazem, which is known to cause atrio-ventricular blockade and bradycardia (risk factors for QTc change; De Ponti *et al*, 2002); a history of hypertension, ischaemic heart disease, myocardial infarction and cerebrovascular accident, reflecting the presence of clinically significant heart disease (another risk factor for QTc change; De Ponti *et al*, 2002); concurrent Lewy body dementia, which is associated with autonomic failure (McKeith, 2000) and frontal lobe deficits that may influence QT intervals (Kubota *et al*, 2001).

My review of the cholinesterase inhibitors (Inglis, 2002) included an analysis of 2791 patients involved in pivotal studies of rivastigmine in Alzheimer's disease (Morganroth *et al*, 2002). About 30% and 10% of these patients had cardiovascular disorders and heart rate/rhythm disorders, respectively. About 35% were receiving concomitant cardiovascular treatments. Even in this relatively at-risk population, heart rate, PQ, PR, QT and QRS intervals were very similar in rivastigmine- and placebo-treated patients, indicating that rivastigmine did not produce adverse effects on cardiac function as assessed by ECG. The lack of cardiac effects associated with rivastigmine may be explained by its selectivity for central over peripheral cholinesterases, and an apparent brain-region selectivity that may avoid areas such as the medullary cardiorespiratory nucleus (Enz *et al*, 1993).

Case reports are an important means of communicating clinical observations. However, it is important that the facts are presented clearly to allow a balanced judgement on the available evidence. I would suggest that the prolonged QTc described in this single case report is more likely to

be due to the confounding factors described above than to a causal association with rivastigmine treatment. The cholinesterase inhibitors form an invaluable part of our limited armamentarium in managing patients with dementia. It would be unfortunate if patients who might benefit from these treatments were deprived of them because of false-positive associations with cardiotoxicity.

#### Declaration of interest

F.I. has conducted research for and been supported by research grants from Janssen-Cilag, Novartis Pharmaceuticals and Shire Pharmaceuticals. He has lectured for Janssen-Cilag and is a member of the Novartis Speakers Bureau.

#### De Ponti F. D., Poluzzi, E., Cavalli, A., *et al* (2002)

Safety of non-antiarrhythmic drugs that prolong the QT interval or induce torsade de pointes: an overview. *Drug Safety*, **25**, 263–286.

#### Enz, A., Amstutz, R., Boddeke, H., *et al* (1993)

Brain selective inhibition of acetylcholinesterase: a novel approach to therapy in Alzheimer's disease. *Progress in Brain Research*, **98**, 431–438.

Inglis, F. (2002) The tolerability and safety of cholinesterase inhibitors in the treatment of dementia. *International Journal of Clinical Practice, Supplement*, **127**, 45–63.

#### Kubota, Y., Sato, W., Tolchi, M., *et al* (2001)

Frontal midline theta rhythm is correlated with cardiac autonomic activities during the performance of an attention demanding meditation procedure. *Brain Research. Cognitive Brain Research*, **11**, 281–287.

#### McKeith, I. G. (2000)

Clinical Lewy body syndromes. *Annals of the New York Academy of Science*, **920**, 1–8.

#### Morganroth, J., Graham, S., Hartman, R. (2002)

Electrocardiographic effects of rivastigmine. *Journal of Clinical Pharmacology*, **42**, 558–568.

#### Walsh, E. & Dourish, J. (2002)

Prolonged QT interval with rivastigmine (letter). *British Journal of Psychiatry*, **180**, 466.

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**Author's reply:** Prolonged QTc interval is defined as a QTc longer than 440 ms (Khan, 2002); therefore, by this definition, the patient did not have a documented prolonged QTc interval prior to the introduction of rivastigmine.

As detailed in the original report of this case to Novartis, the patient had been admitted a number of weeks previously to a medical ward where he developed diarrhoea which was deemed responsible for the lowering of his potassium. As a result he received potassium supplements while

the diarrhoea was ongoing and once the diarrhoea stopped the potassium was rechecked and the potassium supplements were discontinued. The patient had no diarrhoea at any stage during his treatment with rivastigmine that could have led to a further development of hypokalaemia. The patient had been receiving his other medications on a long-standing basis, including diltiazem for 5 years, and electrolytes checked intermittently had not shown previous problems with hypokalaemia. It is therefore unlikely that the patient was hypokalaemic at the time of the prolonged QTc interval.

The patient had no recent history of cardiac abnormalities apart from a myocardial infarct 6 years previously and long-standing hypertension. The patient had been on long-standing medication and there was no evidence of a prolonged QTc while on these medications. Although the patient had symptoms suggestive of dementia with Lewy bodies he did not fulfil the criteria for a diagnosis of probable dementia with Lewy bodies (McKeith *et al*, 1996).

In conclusion, this patient had evidence of a normal QTc interval prior to the introduction of the rivastigmine and developed a prolonged QTc while on the treatment which reverted to normal on discontinuation of the drug. His concomitant medication had been long-standing, he had no recent history of cardiac abnormalities and his previous hypokalaemia secondary to diarrhoea had been corrected. Therefore, we suggest there is a possibility of a causal relationship between rivastigmine and prolonged QTc interval. Independently, Novartis have received two isolated reports of QT interval prolongation, which the company have attributed to confounding factors such as co-medication and electrolyte abnormalities as well as insufficient/discrepancies in documentation (J. Collins (Novartis), personal communication, 2001).

I agree with Dr Inglis that the cholinesterase inhibitors are an invaluable part of our limited armamentarium in managing people with dementia but as with any new treatment only when a large number of patients are treated, many of whom will be taking multiple medications, have different comorbidities and be subject to other conditions that were not represented in the original trial population, will adverse effects become manifest that were otherwise not recognised, appreciated or expected. It is important that clinicians

monitor, document and report adverse events. Unfortunately, experience demonstrates that this is frequently lacking and can result in the delayed recognition of potentially serious side-effects and interactions.

**Khan, I. A. (2002)** Clinical and therapeutic aspects of congenital and acquired long QT syndrome. *American Journal of Medicine*, **112**, 58–66.

**McKeith, I. G., Gelasko, D., Kosaka, K., et al (1996)** Consensus guidelines for the clinical and pathologic diagnosis of dementia with Lewy bodies (DLB): report of a consortium on DLB international workshop. *Neurology*, **47**, 1113–1124.

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### The antidepressant debate should move on

In her editorial Moncrieff (2002) ignored decades of work and focused on a few pieces of research, one of them from 1965. The editorial was followed by a letter criticising this view (Malt, 2002), which was, however, published under the title 'The antidepressant debate continues'. This title might leave the impression that the effectiveness of antidepressants is still questionable.

Some of our colleagues might conclude that antidepressants have no proven effect and their patients should discontinue them. The consequences of such actions have been researched extensively: the relapse rates are approximately twice as high for patients who stop their medication in the first 2–6 months beyond the point of remission, compared with those who continue treatment (e.g. Anderson *et al*, 2000; Hirschfeld, 2001). Other patients might be denied an effective treatment. Going through all the evidence, which includes comparisons with other treatments and between different classes of antidepressants, animal work, and tryptophan and noradrenalin depletion experiments in people responsive to antidepressants, would be like reinventing the wheel, and is not the subject of this letter. As the rest of us continue to learn of advancements being made to refine and improve the pharmacotherapy of depression, is it possible that there is a group believing that antidepressants really do not have an effect? There is indeed an antidepressant debate – but it is not whether they work but rather how they work that is the current focus of interest.

### Declaration of interest

G.K. and A.K. have received grants from Janssen Research Foundation and Glaxo-SmithKline for conducting molecular genetic studies in psychiatric disorders. G.K. has received honoraria from Janssen Cilag for delivering lectures.

**Anderson, I. M., Nutt, D. J. & Deakin, J. F.W. (2000)** Evidence-based guidelines for treating depressive disorders with antidepressants: a revision of the 1993 British Association for Psychopharmacology guidelines. *Journal of Psychopharmacology*, **14**, 3–20.

**Hirschfeld, R. M. A. (2001)** Clinical importance of long-term antidepressant treatment. *British Journal of Psychiatry*, **179** (suppl. 42), s4–s8.

**Malt, U. F. (2002)** The antidepressant debate continues (letter). *British Journal of Psychiatry*, **181**, 531.

**Moncrieff, J. (2002)** The antidepressant debate. *British Journal of Psychiatry*, **180**, 193–194.

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In her editorial, 'The antidepressant debate', Moncrieff (2002) provocatively questioned the orthodox view that antidepressants are efficacious (i.e. work under clinical trial conditions) in the treatment of depressive illness. Questioning accepted views is valuable but Moncrieff missed the real question, which relates to effectiveness, that is when are antidepressants useful clinically? The efficacy argument at the head of her critique, based on individual, often old and poor-quality, studies flies in the face of consistent findings of antidepressant efficacy in systematic reviews and meta-analyses (e.g. Anderson *et al*, 2000). Even the argument of bias due to unblinding because of side-effects is contradicted by her own meta-analysis, which showed a significant benefit for antidepressants over 'active' placebo (Moncrieff *et al*, 1998). Even more compelling is the evidence from continuation/maintenance studies which show that antidepressants have a robust effect in reducing rates of relapse and recurrence (Carney *et al*, 2001), a cumulative effect over months or years. Explaining this by a placebo effect is difficult to accept, or else demands re-evaluation of the nature of placebo.

This is not to say that 'negative' studies, where antidepressants are no better than placebo, should be ignored. An important factor is probably related to severity of depression. Khan *et al* (2002) found that the

proportion of studies favouring antidepressants over placebo increased with the severity of depression; the response to placebo declined with increasing severity whereas that to antidepressants increased. This raises the fundamental question of when (i.e. at what severity) in real life practice does someone with depression clearly benefit from antidepressant drug treatment. Put another way, is the current trend to wider use of antidepressants for milder depression justified? This can only be answered empirically in appropriate naturalistic trials, and even then will require value judgement about the size of the benefit.

Perhaps the most worrying aspect of Moncrieff's editorial was the implication that we should take either a psychosocial or a physical approach to the treatment of depression. Surely we should have put this rather tired dualist view of psychiatry behind us by now? A holistic view combining drug and psychological treatments is to be preferred and evidence is accumulating that this leads to better outcomes. To conclude, a balanced view of the evidence for antidepressants firmly places them as an established and important therapeutic option (alongside others) in the treatment of depression, with their role becoming more central with increasing severity. The true debate is about the best way to use them.

### Declaration of interest

I.M.A. and P.M.H. have both received honoraria for speaking, been members of advisory boards, received research grants and had support for attending scientific meetings from several pharmaceutical companies involved in the manufacture and marketing of antidepressants.

**Anderson, I. M., Nutt, D. J. & Deakin, J. F.W. (2000)** Evidence-based guidelines for treating depressive disorders with antidepressants: a revision of the 1993 British Association for Psychopharmacology guidelines. *Journal of Psychopharmacology*, **14**, 3–20.

**Carney, S., Geddes, J., Davies, C., et al (2001)** Duration of treatment with antidepressants (Cochrane review). *Journal of Psychopharmacology*, **15** (suppl.), A10.

**Khan, A., Leventhal, R. M., Khan, R. K., et al (2002)** Severity of depression and response to antidepressants and placebo: an analysis of the Food and Drug Administration database. *Journal of Clinical Psychopharmacology*, **22**, 40–45.

**Moncrieff, J. (2002)** The antidepressant debate. *British Journal of Psychiatry*, **180**, 193–194.

—, Wessely, S. & Hardy, R. (1998) Meta-analysis of trials comparing antidepressants with active placebos. *British Journal of Psychiatry*, **172**, 227–231.

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**Author's reply:** I would like to make some comments on the points raised by Kirov & Korszun and Anderson & Haddad. They both cite evidence from continuation and maintenance studies, but this is likely to be more flawed than evidence from acute treatment studies. In studies of long-term treatment, patients who have responded to acute treatment are randomised to continue active drugs or to be withdrawn to an inert placebo. However, it cannot be assumed that the state of having had treatment withdrawn is equivalent to never having had treatment in the first place. It is known that there is a discontinuation reaction with all classes of antidepressants (Haddad *et al*, 1998). The symptoms of this reaction may themselves be mistaken for relapse, or they may unblind participants and predispose them to relapse because of fears of discontinuing treatment. This is likely to be a particular problem given that the initial sample of patients comprises people responsive to treatment who are therefore likely to have high expectations of the benefits of treatment.

In addition, the evidence on antidepressant effects and severity is complex. The majority of studies that show that increased efficacy correlates with increased severity are studies of out-patients. In in-patients, more-severe depression has been shown to respond less well to antidepressants than moderate depression does, independently of the presence of psychotic symptoms (Kocsis *et al*, 1990). In our meta-analysis we found no significant differences from placebo in in-patient studies (Moncrieff *et al*, 1998), which is in line with results from other large landmark in-patient studies such as the Medical Research Council study and the National Institute for Mental Health study described in my editorial (Moncrieff, 2002).

Finally, if the benefits of antidepressants are so obvious, it seems surprising to me that we have little evidence that the burden of depressive illness is reducing in line with the vast expansion in antidepressant prescribing. In contrast,

long-term incapacity related to depression has been rising rapidly both in absolute terms and in relation to other conditions (Moncrieff & Pommerleau, 2000).

**Haddad, P., Lejoyeux, M. & Young, A. (1998)** Antidepressant discontinuation reactions. *BMJ*, **316**, 1105–1106.

**Kocsis, J. H., Croughan, J. L., Katz, M. M., et al (1990)** Response to treatment with antidepressants of patients with severe or moderate nonpsychotic depression and of patients with psychotic depression. *American Journal of Psychiatry*, **147**, 621–624.

**Moncrieff, J. (2002)** The antidepressant debate. *British Journal of Psychiatry*, **180**, 193–194.

— & **Pommerleau, J. (2000)** Trends in sickness benefits in Great Britain and the contribution of mental disorders. *Journal of Public Health Medicine*, **22**, 59–67.

—, **Wessely, S. & Hardy, R. (1998)** Meta-analysis of trials comparing antidepressants with active placebos. *British Journal of Psychiatry*, **172**, 227–231.

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### Talking about cognitive analytic therapy

Isaac Marks' review (Marks, 2003) encapsulates the reciprocal roles expressed in so much of the comparative debate in psychotherapy: dismissing:dismissed, contemptuous:contemptible. To contemptuously attack the review would simply be to continue the dance and to encourage further polarising responses. I have great respect for Isaac Marks' work and would invite him to join in a dialogue with cognitive analytic therapy. It was thought-provoking to consider the role of Pavlov in the developmental understanding of symptoms.

Cognitive analytic therapy has its devotees among therapists and clients. It is a tremendously human therapy where the strengths of cognitive theory and object relations theory have more recently begun to incorporate strikingly original ideas on human development, dialogue and the construction of interpersonal meaning from the Russian tradition. For many this represents an exciting evolution of thought concerning the nature of the psychotherapeutic relationship and the process of change in psychotherapy.

Cognitive analytic therapy has attempted to integrate the cognitive and the analytic as well as the dialogic Eastern approach

to development with the reductionist Western scientific tradition. A more challenging task is to bring into dialogue the entrenched culs-de-sac of psychotherapy theory and their defenders. So, let's start to talk and engage in some positive role-play – valuing:valued, respecting:respected, giving:receiving.

### Declaration of interest

J.H. is a member of the Association for Cognitive Analytic Therapy and has published in the field.

**Marks, I. (2003)** Book review: *Introducing Cognitive Analytic Therapy. Principles and Practice*, by A. Ryle & I. B. Kerr. *British Journal of Psychiatry*, **182**, 179–180.

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### Preserve psychoanalysis from too much neuroscience

Professor Hobson (2003) argues admirably for the continued relevance of psychoanalysis in a mainstream psychiatric journal. But is his suggested rapprochement between psychoanalysis and contemporary neuroscience really desirable?

Contemporary neuroscience as illustrated by his example of 'mirror neurons' typically assumes an 'empiricist' worldview. In brief, imitation is assumed to be an acquired process in which information is abstracted from experience using associative learning. The current focus is on the anatomical location of the associative learning responsible for imitation (Rizzolatti *et al*, 2001).

In contrast, psychoanalysis derives from an older, rationalist philosophical tradition. It assumes the existence of both innate beliefs, such as persecutory anxiety, and distinct mental mechanisms, such as introjection or Klein's paranoid-schizoid position, that do not rely on associative learning.

These two philosophies have been in tension for centuries. One option is to make psychoanalysis more empiricist by downplaying the innateness and divergent mental mechanisms of classical theory. This is seen in attempts to incorporate 'theory of mind' deficits into a psychodynamic understanding of mental states (Fonagy, 1991).

But will associative learning form the secure basis for understanding the mind that empiricism proposed? Practical

attempts to develop this philosophy, such as neural networks, are not actually very good at accounting for how we manage to form and fix true beliefs. Rationalist accounts of higher brain functions such as language do exist (Chomsky, 1959). Given such uncertainty rationalism continues to merit the foothold within psychiatric practice that psychoanalysis provides. Too much mutual understanding between psychoanalysis and contemporary neuroscience might not be good for psychiatry.

#### Declaration of interest

H.J. has previously received unrestricted educational grants from Eli Lilly and Astra-Zeneca.

**Chomsky, N. (1959)** A review of Skinner's verbal behaviour. *Language*, **35**, 1. Reprinted (1980) in *Readings in Philosophy of Psychology, Vol. 1* (ed. N. Block). Cambridge, MA: Harvard University press.

**Fonagy, P. (1991)** Thinking about thinking: some clinical and theoretical considerations in the treatment of a borderline patient. *International Journal of Psychoanalysis*, **72**, 639–656.

**Hobson, R. I. (2003)** Between ourselves: psychodynamics and the interpersonal domain. *British Journal of Psychiatry*, **182**, 193–195.

**Rizzolatti, G., Fogassi, I. & Gallese, V. (2001)** Neurophysiological mechanisms underlying the understanding of imitation and action. *Nature Reviews Neuroscience*, **2**, 661–669.

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## One hundred years ago

### Terrible fire at Colney Hatch Asylum

A fire, attended with the most disastrous consequences and involving a fearful loss of life, occurred early yesterday morning at Colney Hatch Lunatic Asylum, the large hospital for the pauper insane belonging to the London County Council, and situated at New Southgate...

A few minutes after half-past 5 yesterday morning the steam siren at the asylum sounded the fire alarm, and the inhabitants of New Southgate, Barnet, and Edmonton, the parishes surrounding the asylum, who swarmed into the streets, saw a startling glare showing from the asylum grounds. It was evident that a disastrous fire, which had already obtained a strong hold, was in progress. A number of local residents climbed the wall of the asylum at the rear with a view of rendering assistance, but their aid was refused. The fire which had broken out so suddenly and was destined to end so tragically began at the bottom block of the temporary wards. It burnt from the outset with great fury, and in a few seconds the whole of the southern

block, known as X ward 5, was involved. The buildings, being erected on timber frames and lined with matchboarding, of course fed the flames, and there being a high wind blowing at the time, every element necessary to assist the blaze was present. The asylum house fire brigade at once resolutely attacked the fire, but apparently they were in difficulties owing to the lack of water, and they were also short-handed for a task of such magnitude as that which confronted them, there being less than a dozen of the asylum staff drilled as firemen resident inside the walls. The heat and smoke created by the fire were also bad elements to contend with, it being impossible to approach the burning block. In these circumstances it was not surprising to the spectators to observe after a very few minutes that X Ward 4 had burst into flames, which had swept along the communicating corridor, meeting with no opposition, while by this time the iron sides and roof of X 5 were almost at a white heat.

The Hornsey Fire Brigade had been the first to get their steamer to work, but they were unable to do any effective duty until they had dammed a brook at the bottom

of the slope, about 400 yards from the fire. When they began to play upon the flames it was too late to prevent the total destruction of the temporary wards, which, in little more than an hour after the outbreak was discovered, had been burned out from end to end and had crumpled down. One after another the doomed huts burst into flames. For a while each burned with a brilliant glare, the flames shooting high into the air through the slightly-constructed roof. Then the roof and walls collapsed amid a shower of sparks, and the fire swept on to claim its neighbours. One by one in this rapid way all five of the wards tumbled down, a heap of smouldering ruins.

When day dawned, while some of the firemen pumping water from the brook below continued to play on the red hot *débris*, others began the terrible task of searching the ruins. Then it was discovered that the fire had claimed many victims...

#### REFERENCE

*The Times*, 28 January 1903.

Researched by Henry Rollin, Emeritus Consultant Psychiatrist, Horton Hospital, Epsom, Surrey

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